

Discussions

MICHAEL FARADAY : A BIOGRAPHY

Professor Kuhn's criticisms¹ deal with three areas: on the most elementary level, he finds that I have either mis-read, misunderstood, or neglected certain basic scientific texts. He also questions my attribution of certain of Faraday's ideas to the influence of Kant and Boscovich and, finally, he notes that the sociological and the psychological dimensions of Faraday's life are almost entirely absent. There is, I think, a discernible bias running through all these criticisms of which even Kuhn may not be aware, and this is my reason for replying to his review. There are also some real areas of disagreement which may affect the future of the history of nineteenth-century science and which, I feel, deserve to be made quite explicit.

The charge of inaccuracy or 'careless reporting' is easily met for with one exception it is Kuhn who is in error. His 'pure' example of Ritter's double V tubes may serve here to illustrate this. He need not have gone back to Ritter's original paper to 'discover' the acid in the V tube; this is clearly stated on page 228. Indeed, the description of the experiment is exact. Kuhn's 'solution', incidentally, happens to be wrong. He feels that the presence of acid (in this case, sulphuric acid) removes all difficulties, both for the modern reader and for Ritter's contemporaries. Sulphuric acid, however, in the early 1800s was *not* H_2SO_4 but SO_3 and could not, therefore, have supplied the hydrogen. Georges Cuvier, in his report on Ritter's experiments to the Académie des Sciences, wrote: '[Galvanic action] decomposes the water, permitting the disengagement of one of the gases at the end of the wire, and conducting the other gas in some invisible manner to the end of the other in order to release it there,' (quoted on p. 230). If this statement is applied to Ritter's experiment, which Cuvier certainly knew, it necessarily implies the passage of the hydrogen through the connecting wire of the V tubes.

The other 'errors' Kuhn finds are of exactly this sort. On Volta, Kuhn neglects to inform his readers that there is a direct quotation from Volta which says precisely what I said Volta said. This quotation also explains the figure mentioned by Kuhn. It might be noted parenthetically here that the figures of which Kuhn complains are, page 152, an exact copy of the figure used to illustrate Wollaston's theory; page 157, an exact copy of Faraday's sketch. His trouble with primary sources here merely means that they *are* sometimes hard to follow. The figure on page 425 is a difficult one, and I must apologise for its obscurity.

Again, on Faraday's enunciation of Ohm's law, Kuhn is less than frank with his reader. Faraday says a good deal more than, 'the tendency to generate a current must be directly as [the conducting] power' and what he says is given in a quotation on page 210. It may be summarised as follows: given equal

¹ This *Journal*, 1967, 18, pp. 148-154.

inducing (or electromotive force) the currents produced by magneto-electric induction in bodies are *exactly* proportional to their conducting power, with conducting power defined as the inverse of what Faraday calls the obstruction. I leave it to the reader to decide if this is Ohm's law.

I plead guilty to the error on Ampère. I should not have stated that 'the rationale of these experiments was clearly given by Ampère's theory', but, rather, that the rationale could be deduced from Ampère's theory, provided Faraday did not insist upon the absolute distinction between electrostatic and electrodynamic effects. The evidence presented on page 173 indicates that Faraday was not as conscious of this distinction as is Kuhn.

Kuhn further charges me with neglecting the works of contemporaries, such as Poisson, and here again I shall plead guilty. This time, however, my guilt is consciously self-imposed. I also did not discuss the mathematical aspects of Ampère's theory, the magnetic work of Gauss and Weber, and a whole host of other topics. My reasons are simple and, I would have thought, acceptable to Kuhn. I set out to write a biography of Faraday and the problem I set myself was to get inside Faraday's head, to see the questions that presented themselves to him, and to try and discover how he went about attacking and answering them. My discussion of the works of others, therefore, was limited to that part of it which Faraday read and understood. The problem of the nature of the voltaic cell, for example, upon which Kuhn places such emphasis is nowhere to be found in Faraday's early musings. Similarly, Poisson's achievement, of which Kuhn accuses me of being unaware, is irrelevant to a consideration of Faraday's intellectual development. Thus, I would maintain, that the 'pattern' which I am accused of reversing is, in fact, the same throughout the book. It is the pattern of Faraday's mind in so far as I could capture it. What I have not done is written a history of electricity and magnetism in the nineteenth century and Kuhn taxes me for this. I can only ask him to have patience for it is a future project.

The problem of the philosophical influences on Faraday is, as I have admitted in the book, a difficult one. My treatment has been uniformly criticised because of the paucity of evidence. All I can do here is point out that my hypothesis of the influence of Kant and Boscovich does serve to give coherence to Faraday's work where none was ever discerned before. As Charles Darwin wrote, 'In scientific investigations it is permitted to invent any hypothesis, and if it explains various large and independent class of facts it rises to the rank of a well-grounded theory' (C. Darwin, *The Variation of Animals and Plants under Domestication*, 2 vols., London, 1868, 1, 8). I claim no more for my hypothesis, hoping that further evidence will either confirm or refute it. May I, incidentally, suggest here that the fact that *Naturphilosophie* 'is now increasingly recognised as a vital formative element in nineteenth century scientific thought' owes something to my treatment of its influence on Faraday.

My case for the influence of Boscovich on Faraday rests upon far more than Kuhn states. My 'failure' to mention his belief in point atoms before 1844 simply does not exist. There is an extended section on Davy and Boscovich, drawn from Davy's writings; there is mention of Faraday's early enthusiasm for Thomas Thomson's textbook of chemistry in which Boscovich is given extended treatment; there is Faraday's mention of the theory of point atoms in his early chemical lectures (1819). Since I am in the process of preparing a paper on this

topic, I shall not here spell out my arguments and further evidence for the connection between Faraday and Boscovich's ideas. There is certainly more in the book than Faraday's 1844 statement.

Finally, Kuhn objects to my failure to provide a sociological and psychological environment for Faraday. He appears to assume that Faraday *must* have been influenced by his milieu and by his friends. I agree with Kuhn that one should look for such a sociological medium for it *may* be important in the understanding of a man's thought. It is, after all, in the sociological context that the full force of the Kuhnian paradigm may be brought to bear. What Kuhn appears unwilling to concede is that there may *not* be a sociological context. Faraday was a loner. His scientific acquaintances were just that—acquaintances who were never admitted to the intimacy in which Faraday lived with his Sandemanian brethren. If Kuhn says that this point does not emerge in the book, I can only take his word for it, but the point is there although it is not insisted upon. Where there is evidence for influence, as with Davy and John Herschel, the influence, I maintain, is clearly delineated.

The degree to which the book captures the essence of Faraday's personality will have to be decided by each reader. I think the key phrase in Kuhn's critique here is 'plausible to a twentieth century reader'. The evidence we have of Faraday's personality reveals him as kind, serene, proud, gentle, honest and simple, in the Victorian sense of the word. These are without exception the qualities attributed to him by his contemporaries. Since Freud, of course, it is impossible to believe that anyone could *really* be like this and so we are expected to detect the seething volcano which *must* exist under this calm exterior. I classify psychoanalysis with humoral pathology and would as soon describe Faraday as phlegmatic or jaundiced as set off on a psychoanalytic voyage *for which there is no guide whatsoever* in the sources. Nor is Kuhn much help here. How, he asks, can the simple man I have described be 'also the man who was repeatedly concerned to establish his priority in scientific discovery'. There is no problem at all, for Faraday never was repeatedly concerned to establish his priority. In 1821 and 1823 Faraday wished to establish his claim for originating the idea of electromagnetic rotations because he had been accused of plagiarism. In 1831, Nobili and Antinori published an account of electromagnetic induction experiments actually based on Faraday's great discovery. There were some, however, who did not read carefully and thought that Nobili and Antinori had discovered electromagnetic induction before Faraday. Faraday was quick to point out that he was the source so that another charge of plagiarism could not be made. I do not know the degree of complexity necessary to be sensitive to the accusation of plagiarism but I suspect it is not large. That Faraday was not obsessed by priorities is easily shown: in 1845, for example, when informed that others before him had discovered the peculiar action of bismuth in a magnetic field, he added a note to his paper giving them credit. This example could be multiplied. Indeed, these incidents seem to me to buttress the very depiction of Faraday's character given in the book. He may have been concealing a volcano but even Kuhn must confess that he did a wonderful job of it—so good, in fact, that there is not a shred of evidence to indicate that it existed.

This rather detailed rebuttal of Kuhn's criticisms was not solely intended as a defence against the serious charges of sloppy scholarship which Kuhn has

levelled against me. It was also meant to point out the position which Kuhn has assumed—one which follows almost automatically from his well-known philosophical views. Kuhn, it seems to me, is essentially hostile to the idea of biography as the proper mode of the history of science. The scientific maverick (and many of the truly creative scientists seem to me to fall in this class) is immune to the siren song of the paradigm. I think Kuhn's disappointment with my biography of Faraday comes, in large part, from the fact that no matter how hard he tried to push Faraday into the scheme which he feels reveals the true dimensions of the history of science, Faraday would not fit. Thus the pique at the failure to discuss the Royal Institution as a unique scientific institution, and the sharp language on the failure to sketch in the voltaic and electrodynamic paradigms and so on. If there are dangers in the biographical approach as I have used it, it seems equally clear that there are also dangers in the kind of history of science that Kuhn wishes I had written but did not.

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Professor Kuhn replies:

I am grateful for Professor Williams's attempt to clarify the issues that divide us. If it leads readers to examine his book and reach their own conclusions, both of our purposes will have been achieved. I shall here only insist that, where particular passages of his book are at issue, our differences are not at all the product of misreading or of incomplete reporting in my review.